Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-432-RC1, 2018

Interactive comment on "Dimethylsulfide dynamics in first-year sea ice melt ponds in the Canadian Arctic Archipelago" *by* Margaux Gourdal et al.

Anonymous Referee #1

Received and published: 4 January 2018

Review for "Dimethylsulfide dynamics in first-year sea ice melt ponds in the Canadian Arctic Archipelago" by Gourdal and co-authors

General comments: This is a generally well written and interesting manuscript describing novel measurements of Dimethylsulfide (DMS, DMSPd and DMSPp) concentrations and dynamics (derived from labelled DMSP and DMSO isotopic marker incubations) in Arctic sea-ice melt ponds. A shortcoming of the paper is that it is based on a rather limited dataset with consequent problems for statistical analyses. Only two (brackish) melt ponds very sampled for incubations in this study, and statistics are based on an N=2 (with additional duplicate - but apparently dependent - samples taken

from each incubation). While a t-test can be employed for a dataset with an N=2(4), the dataset appears extremely small to make any statistical relevant conclusions. This reviewer therefore suggests to clarify (provide df or define N values) or alternatively delete these statistical analyses and rephrase some of the statements in relation to DMSP transformation into DMS. This said, other methods applied in this study appear to be solid (noting that this reviewer is not an expert in GC/GC-MS DMS(P) analyses) and raise some important new research questions for future research on DMS dynamics in sea-ice melt ponds. In summary this reviewer suggests publication of the manuscript after amending the statistical analyses (t-test) and some other (minor) shortcomings including a re-consideration of the estimate of the overall DMS reservoir in Arctic melt-ponds, and a more detailed discussion on the sea-ice surface permeability.

Specific comments:

P2, L1: delete "natural" in first sentence of abstract, this word is not needed

P2 , L 12: This calculation of the DMS reservoir in Arctic melt-ponds is based on 2 single measurements of 2 very specific (=brackish) melt ponds in a very defined study area (e.g. the Canadian Archipelago). This reviewer considers up-scaling the results from this study to the entire Arctic as highly problematic. It is suggested to delete this estimate from the manuscript (see also page 16) or at least to delete this broad-brush estimate from the Abstract.

P4, L 17: No need to start a new paragraph

P4, L32: be more specific: . . .of melted ice samples" rather than "melt water samples"

P5, L1: The T and S data from the 10 cm surface ice allow the accurate calculation of the brine volume fraction according to established formulas, see e.g., Eicken, H., H. R. Krouse, D. Kadko, and D. K. Perovich, Tracer studies of pathways and rates of meltwater transport through Arctic summer sea ice, J. Geophys. Res., 107(C10), 8046, doi:10.1029/2000JC000583, 2002; an references therein. Applying these formulas (e.g. those for high T and low S sea ice values, e.g. Manninen and Leppaeranta 1988, cited in above reference), and using the values reported in the manuscript of T = -0.2C and S = 0 psu actually indicates “im”-permeable ice, while a T = -0.2C and S = 0.8 psu indicates a brine volume of about 20% (= highly permeable ice). This reviewer suggest that brine volumes are calculated for the T – S measurements and that a more detailed discussion on ice permeability/sea water percolation is given. Please also note that a) “the rule of 5s” is primarily based on a brine volume fraction of 5% (which can be achieved by different T-S combinations, including T = -5C and S = 5, b) that this percolation threshold is only valid during thermodynamic equilibrium, and c) also only applies for columnar ice (likely the case in these samples), but this surface ice might have also undergone some melting/metamorphosis). In summary this reviewers suggest a more detailed discussion of the sea ice permeability. The current conclusions are fine, but just stating “according to the rule of 5s” is insufficient.

P4, L 11: . . ..replicates. . . How many?

P4, L25: This reviewer suggest to add a sentence and a definition of “HNA” here, e.g. what nucleic acid stain was used in this fly cytometry protocol?

P6, L23: It is unusual to refer to PAR as “700-400”, normally one would write “400-700”. This also applies to the UVA and UVB wavelengths given in the text.

P 8, L25: As discussed above, this reviewer suggests to revisit the t-test statistics applied: It appears that N equals 2, which makes application of the t-test problematic. At least more explanation is needed.

P 9, L3: This reviewer suggest to use the SI unit “m” rather than “cm” as unit for length measurements throughout the manuscript/figures.

P9, L7: As per above more details is required than just stating the “rule of fives”.

P9, L 16: use singular, e.g. “detail”

P11, L10 -15: If “significantly” is used test-statistics should be given, also provide df value and/or N. Given the low N, these statistical results are of little relevance.

P 12, L 17: Sea “spray” rather than “spay”

P12, L28: Here “gravity drainage” and “brine flushing” are used to describe the same process, while classically “brine drainage” refers to the release of cold salt brines in surface-cooled sea ice, while “brine flushing” refers to the flushing out of salt through meltwater, e.g. they are technical terms used for different physical processes.

P 13, L 10: No data are shown that demonstrate :”full depth desalinization” -> please clarify

P 13, L 20: Avoid the use of “significant” if no statistical test was conducted /or provide statistical results.

Fig and Tables:

Fig 3: Unusual numbering of panels: “c” should be “b” and “b” should be “c”?

Tab 7: “control” or “Control” -> consistency in spelling needed

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